Letters

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Impact of surgery for stress incontinence on morbidity

Effects of confounding variables on outcomes of incontinence surgery must be considered

EDITOR—Black et al's study highlighted difficulties that those who assess the treatment of urinary incontinence may encounter.[1] The different surgical procedures were considered together, which makes it impossible to interpret cure rates or postoperative morbidity. Previous continence surgery was not reported, although this affects the results of surgery.

Black et al state that calculations of sample size were inappropriate as the objective was to provide preliminary estimates of variables and to generate hypotheses. They found no significant difference in outcome between women who had preoperative urodynamic testing and women who did not. On the basis of the objective cure rates reported in the literature,[2 3] however, two groups of 220 women would be needed to show that urodynamics does not make a difference to the outcome of surgery, assuming a power of 0.90 and a significance level of 0.05.[4] Black et al's study is too small to show this and does not mention the type of testing or the reasons why some women had tests and others did not. Those who did may have had previous continence surgery, which is known to reduce the cure rate.

The statement that urgency and urge incontinence should not be considered contraindications to surgery has no basis from this study. Unfortunately, 93% of women with detrusor instability have these symptoms, and they would not benefit from a continence procedure. The women who were not operated on after urodynamics have not been mentioned.

In Black et al's study only 28% of women were found to be continent one year after surgery. This contrasts with a symptomatic follow up study carried out at our institution on women who had undergone preoperative urodynamics and then had continence surgery performed by us or the original referrer. We assessed 201 women with a postal validated disease specific questionnaire before and after colposuspension.[5] Only 19% of the women had incontinence, and 12% found that this disturbed their lifestyle to some degree. Of a smaller number of women (28) who had undergone an anterior repair, only 36% were dry. The greatest contrast between the data was that 97% of the women we treated would recommend investigation and treatment to their friends, compared with 68% in the reported study.

The effects of incontinence surgery should be studied with an understanding of the confounding variables that can affect findings; otherwise the results are not meaningful. The data presented by Black et al may be misleading to those who perform and those who undergo incontinence surgery.

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1 Black N, Griffiths J, Pope C, Bowling A, Abel P. Impact of surgery for stress incontinence on morbidity: cohort study. *BMJ* 1997;315:1493-8. (6 December.)
2 Stanton SL, Cardozo LD. Results of the colposuspension operation for incontinence and prolapse. *Br J Obstet Gynaecol* 1979;86:693-7.
3 Colombo M, Zanetta G, Vitobello D, Milani R. The Burch colposuspension for women with and without detrusor overactivity. *Br J Obstet Gynaecol* 1996;103:255-60.
4 Altman DG. How large is a sample? In: Gore SM, Altman DG, eds. *Statistics in practice*. London: BMA, 1982:6-8.
5 Kelleher CJ, Cardozo LD, Khullar V, Salvatore S. A new questionnaire to assess the quality of life of urinary incontinent women. *Br J Obstet Gynaecol* 1997;104:1374-9.

Patients should be told hospital results and allowed to choose where they want surgery

EDITOR—Black et al report success rates for stress incontinence surgery that are significantly below those reported in the literature.[1] There are several possible explanations for this finding.

Only 64 of 137 eligible gynaecologists and urologists were willing to participate in Black et al's study, and just 49 (36%) took part. Among 631 eligible patients, outcome data (obtained through a questionnaire) were available for only 367 (58%). Information about the impact of preoperative assessment was available in only 285 cases (45%), with just 164 (29%) having urodynamic studies.

Three separate operations were studied and were reported together, one of which (anterior colporraphy) is known to have poor results and should no longer be considered appropriate for the treatment of genuine stress incontinence.[2] The International Continence Society defines urinary incontinence as urinary leakage that can be shown and leads to a social or hygienic problem.[3] Black et al report low rates of success (continence) on the basis of their own severity index, which seems sensitive but does not clearly measure the effect of incontinence on a woman's quality of life ("bother factor"). Altogether 18% leaked only once a month or less and, provided this was not a problem to them, treatment should not be considered to have failed in such patients.

The series in the literature are published by surgeons who use urodynamics to indicate the need for and type of surgery. Additional information on bladder and urethral function should be taken into account when counselling women before surgery. We do not know the number of surgeons who believe that they have a special expertise in the management of dysfunction of the lower urinary tract. A doctor's failure to understand the urodynamic report and to offer counselling may explain a patient's lack of satisfaction after surgery.

We agree that patients' symptoms and their views on surgical outcome should be assessed independently. Patients' symptoms can be an unreliable indicator of urinary tract function,[4] and objective evidence of urine loss is therefore important.

The authors say that the high reported success rates of other studies are a result of these being performed in centres of excellence. The aim of tertiary referral centres is not only to set high standards of training and surgical success but also to assist peripheral units in achieving similar results. If other hospitals are unable to maintain such high standards then perhaps the patients should be informed of the differing results and allowed to choose where they have their incontinence surgery performed.

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2 James MC, Abrams P. Recent analysis of surgical outcomes for incontinence. *Curr Opin Urol* 1997;7:215-8.
3 Bates P, Bradley WE, Glen E, Melchior H, Rowan D, Sterling A, et al. First report on the standardization of terminology of lower urinary tract function. *Br J Urol* 1976;48:39-42.
4 Jensen KJ, Nielsen FR, Ostergard DR. The role of patient history in the diagnosis of urinary incontinence. *Obstet Gynecol* 1994;5:904-9.

Treatment needs to be based on objective assessment rather than on symptoms

EDITOR—I am one of the 13 surgeons who declined to participate in Black et al's study because of concerns about the methodology.[1] They state that it is possible to obtain standardised data from patients about the severity of their genuine stress incontinence. Describing loss of urine in quantitative terms such as a "cupful" must be less accurate than performing a pad test. Doctors simply asking about frequency is

not as accurate as patients completing a filling and voiding chart. Using patients' opinions rather than surgeons' opinions is not a substitute for objective assessment.

Black et al review quite diverse procedures. They make no attempt to relate the impact of surgery to the actual operation performed despite the evidence that colposuspension is associated with a better outcome than anterior repair.[2] They give no data about patients in whom surgery had failed previously. We also do not know what proportion of the anterior repairs were performed for prolapse as well as genuine stress incontinence. These two form distinct groups and should be excluded from the study.

In presenting the data obtained by urodynamic assessment, Black et al introduce unfair bias. In their conclusions they discuss the 39% of patients who had surgery without urodynamic testing, implying that 61% were tested. These figures refer only to those for whom the information was available. In fact, no urodynamic information was available for 63% of the overall group who had surgery, which makes any conclusions about the value of the figures void.

Although I accept that surgery may have more limitations than stated in the standard textbooks, Black et al's study has done little to advance the treatment of women with urinary incontinence. It fosters an ethos of treatment based on symptoms rather than objective assessment and choosing a surgical procedure at random rather than opting for the one that is associated with the best outcome.

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2 Jarvis FJ. Surgery for genuine stress incontinence. *Br J Obstet Gynaecol* 1994;101:371-4.

Colposuspension has highest cure rates

EDITOR—Black et al's study showed not only the poor results of surgery for stress incontinence (only 28% women were cured) but the fact that surgeons are still using anterior repairs and needle suspensions as operations (on 44% of the women in this study).[1]

The success of these operations in curing genuine stress incontinence probably approaches 50% in the short term, and even in the hands of specialists this figure is likely to be only 30% in the longer term.[2] This contrasts sharply with colposuspension, which has cure rates that are maintained at 69% for 10-20 years after surgery.[3] Gynaecologists and urologists interested in the field of incontinence recognise that the anterior repair and needle neck suspensions are now considered inappropriate operations for most women with incontinence.[4]

The substandard outcome for these women will be rectified only when each hospital has its own consultant led service for the investigation and management of disorders of the lower urinary tract as suggested by the Department of Health.[5] Until then the poor results detailed in Black et al's study are likely to continue.

Few specialists would argue against the use of urodynamics as an aid to the diagnosis of urinary disorders and incontinence. History and examination are the foundation of assessment, however, and urodynamics are merely a part of this process. Simple investigations such as a fluid balance chart and urine microscopy and culture may be more useful than urodynamics. Specific urodynamic findings are associated with higher surgical failure rates, however, and with increased complications such as voiding disorders. Urodynamic evaluation is essential in every woman contemplating surgery so that the likelihood of specific complications can be estimated. I agree with Black et al that women should be fully informed about their treatments, and this includes the likelihood of specific complications as defined by urodynamics.

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1 Black N, Griffiths J, Pope C, Bowling A, Abel P. Impact of surgery for stress incontinence on morbidity: cohort study. *BMJ* 1997;315:1493-8. (6 December.)
2 Mills R, Persad R, Handley Ashken M. Long term follow-up results with the Stamey operation for stress incontinence of urine. *Br J Urol* 1996;77:86-8.
3 Alcalay M, Monga A, Stanton SL. Burch colposuspension: a 10-20 year follow up. *Br J Obstet Gynaecol* 1995;102:740-5.
4 Stanton SL. What is the right operation for stress incontinence? A gynaecological view. *Br J Urol* 1997;80:84-7.
5 Department of Health. *An Agenda for action on continence services*. London: DoH, 1991. (ML(91)1.)

Pragmatic randomised trial is required

EDITOR—We are concerned about the key messages of Black et al's study—namely, the doubful value of preoperative urodynamic testing, and the lack of predictive importance of urgency and urge incontinence.[1]

Urodynamic testing aims to confirm clinical diagnoses and to identify variables that might alter diagnoses or treatment. It helps to identify women who are unfit for an operation, especially those with an overactive detrusor function. These women are often bothered by urgency and urge incontinence.

Consequently, the population in Black et al's study is subject to selection bias, and conclusions about the two key issues mentioned above are not possible. To study the value of urodynamic testing and of urgency and urge incontinence as predictors requires a pragmatic randomised trial.

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Authors' reply

EDITOR—Our study on the impact of surgery for stress incontinence is challenged on five counts. Firstly, it is suggested that the poor outcomes were partly a result of our inappropriate inclusion of women treated by anterior colporrhaphy. While colposuspension was associated with a greater improvement in the severity of symptoms than was anterior colporrhaphy (index scores at one year follow up 4.7 *v* 7.2), the type of procedure did not predict outcome when confounders were taken into account. Our study was deliberately pragmatic. The aim was to assess the effectiveness of surgery provided in typical clinical practice.

Secondly, Khullar et al and O'Connor rightly point out the lack of adjustment for confounding factors. About one fifth of women had undergone previous surgery, but this was not associated with any outcome. We therefore do not share Khullar et al's view that previous surgery affects results greatly.

Thirdly, Khullar et al think that our sample size was "too small to demonstrate a difference" between the 60% of women who underwent preoperative urodynamic studies and the 40% who did not (difference in severity of symptoms after one year 0.3). All that a larger sample would achieve, however, is a narrowing of the confidence interval around the difference (– 1.2 to 1.8). It would not be expected to change the size of the difference. O'Connor and James are both concerned about our sample being unrepresentative of hospitals, surgeons, and patients. We are unaware of any other study of incontinence that has included such a large number of hospitals and surgeons: of the 31 published prospective studies, 28 were single centre studies and 29 were confined to teaching hospitals.[1] The lack of any serious patient recruitment bias in our study was shown in the study, and any response bias would reinforce rather than detract from the finding that poor outcomes are more common than previously believed.

Fourthly, we do not agree with O'Connor, who believes that "patient reports must be less accurate than performing a pad test," and James, who states that "patients' symptoms are unreliable." One of the principal reasons for operating is to reduce the severity of symptoms. The other, as James says, is to reduce the "bother factor" of the symptoms. This, together with other social outcomes, will be reported in another paper.

Finally, our data do not support Duckett's and James's view of specialist centres. Neither operating in a teaching hospital nor a surgeon's grade and annual volume of operations performed were associated with the outcome of surgery. Similarly, Duckett's view that urodynamic pressure studies need to be carried out in all women is not supported by our data. Like Lose and Walter, we believe that a pragmatic randomised trial is needed to identify the appropriate indications for such tests.

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Risk factors for winter outbreak of acute diarrhoea in France

Winter outbreaks of diarrhoea occur in United Kingdom too

Editor—Letrilliart et al conclude that the winter epidemic of diarrhoea in France in 1995-6 was not associated with consumption of tap water or shellfish, that person to person spread was implicated, and that the epidemic was of viral aetiology.[1] Despite considerable reservations about the validity of their evidence we agree that the epidemic may have been largely attributable to viral diarrhoea.

Winter outbreaks of viral diarrhoea associated with shellfish have been described in the United Kingdom,[2] but the French study's failure to show an association may have been because of the methods used. Infection with small round structured virus, the most frequently identified cause of foodborne viral gastroenteritis, induces short term immunity,[3] which could produce perverse effects in a study of this design—for example, people who eat oysters frequently might be differentially immune owing to higher exposure to risk inducing an apparent protective effect for frequent consumption. Separate analysis of primary and secondary cases would be more appropriate to test the primary hypothesis that food and water consumed were responsible for the epidemic.

The descriptive epidemiology and the design of the study are inadequate to explain the hypotheses selected. The apparent epidemic peaks in the incidence of infectious intestinal disease in January of most years are not clear,[1] and figure 1 suggests that a high incidence occurred over the whole winter. A high intake of shellfish at Christmas and New Year would be expected to cause diarrhoea only in weeks 52, 1, and 2.

Disappointingly, the authors did not address age distribution; age is a crucial explanatory factor in the epidemiology of diarrhoeal illness. The incidence of diarrhoea is much higher in children than adults. During 1996 in the United Kingdom 17 140 episodes of infectious intestinal disease were reported in the weekly return service of the Royal College of General Practitioners; of these, 9283 were in children aged 0-4 and 5267 in children aged 5-14.[4] We estimated that rotavirus was causing roughly three tenths of episodes of diarrhoea in children under 5 who consulted in general practice in the United Kingdom, and these cases showed a distinctive winter peak (figure).[5] No seasonal pattern was apparent in older age groups.

To explain the aetiology of winter diarrhoea, detailed descriptive epidemiology combined with adequate bacteriological and virological investigation of the cases would be a more appropriate approach to assessing the potential risk factors. Such an approach is currently being taken by the Royal College of General Practitioners in collaboration with the Public Health Laboratory Service.

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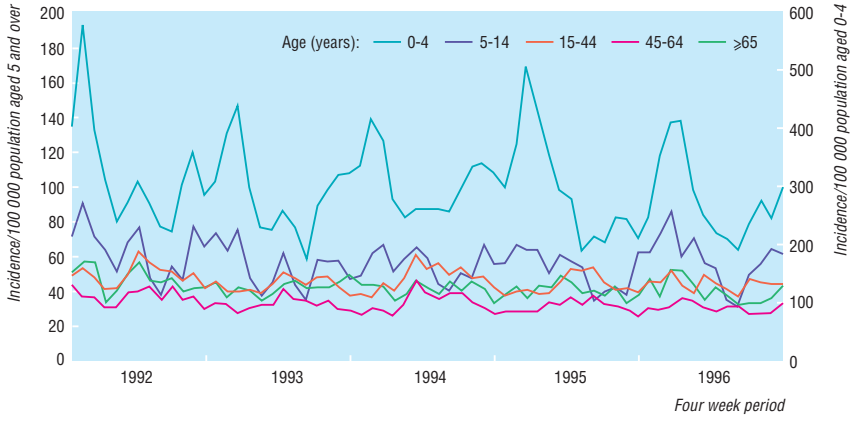
2 Communicable Disease Surveillance Centre. Shellfish and outbreaks of gastroenteritis in England and Wales: 1996 and 1997. *Commun Dis Rep CDR Wkly* 1998;8:21.
3 Parrino TA, Schreiber DS, Trier JS, Kapikian AZ, Blacklow NR. Clinical immunity in acute gastroenteritis caused by the Norwalk agent. *N Engl J Med* 1977;297:86.
4 Fleming DM, Crombie DL, Ross AM. *Annual report of the weekly return service for 1996.* Birmingham: Royal College of General Practitioners, 1997.
5 Djuretic T, Ramsay M, Gay N, Wall PG, Ryan M, Fleming D. An estimate of the proportion of diarrhoeal disease episodes seen by general practitioners attributable to rotavirus in children under five years of age, England and Wales. *Acta Paediatr* (in press).



Mean weekly incidence of infectious intestinal disease seen in general practice, by four week period and age group (from Royal College of General Practitioners' weekly return service, 1992-6)

Authors did not rule out shellfish as a factor

Editor—Letrilliart et al claim to have excluded consumption of shellfish from the factors accounting for the winter outbreaks of diarrhoea in France.[1] We did not find evidence in their paper to support this claim.

Firstly, the selection of cases was biased. The authors should have attempted to include all cases who consulted the doctors throughout the one month study rather than selecting only the first three cases who consulted each doctor.[2] The first three patients with diarrhoea seen by a doctor during an epidemic of diarrhoea are unlikely to be representative of all the patients with diarrhoea seen by the same doctor during one month of the epidemic. Such a sample of cases is even less likely to be representative of cases in the community.

Secondly, the interpretation of the results given in the paper does not support the claim that consumption of shellfish was excluded as the source of the winter outbreaks. The finding that the cases with diarrhoea were more likely to have had contacts with other people with diarrhoea suggests that these cases were mostly secondary cases. Nothing was mentioned about the index cases, who are the key to the "cause" of the outbreak.

Index cases may have consumed raw shellfish and got infected with, for example, a Norwalk-like virus (small round structured virus).[3] Transmission from person to person may have led to the secondary cases. Norwalk-like viruses can be transmitted effectively from person to person as well as through a contaminated food source (such as raw oysters) or water.[3] The authors did not report other clinical symptoms that accompanied diarrhoea in their patients. Vomiting is one of the common symptoms of gastroenteritis due to Norwalk-like virus and the source of the name "winter vomiting disease."

Finally, seasonality of gastroenteritis in the community is not limited to France. In England and Wales, general outbreaks of infectious intestinal diseases (community and institutional outbreaks combined) during 1992-4 peaked during summer.[4] Salmonella infections, the commonest cause of these outbreaks, had peaks in July, while infections with Norwalk-like virus, the second commonest cause, had peaks in October.[4]

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2 Kelsey JL, Thompson WD, Evans AS. *Methods in observational epidemiology.* New York: Oxford University Press, 1986.
3 Viral Gastroenteritis Subcommittee of the PHLS Virology Committee. Outbreaks of gastroenteritis associated with SRSVs. *PHLS Microbiol Digest* 1993;10:2-8.
4 Djuretic T, Wall PG, Ryan MJ, Evans HS, Adak GK, Cowden JM. General outbreaks of infectious intestinal disease in England and Wales 1992 to 1994. *Commun Dis Rep* 1996;6:R57-63.

Authors' reply

EDITOR—We we do not understand what Fleming et al mean by the statement that "the apparent epidemic peaks in the incidence of infectious intestinal disease in January of most years are not clear"; these peaks are clearly shown in figure 1 of our paper, in which the visual impression is supported by state of the art time-series analysis.[1] We agree with Fleming et al that breaking down this figure by age group is common in epidemiology, as the Royal College of General Practitioners does routinely. We do not, however, believe that it was necessary in this paper, which was based on a case-control design; those interested can find this additional approach illustrated in Valleron's paper.[2] That shows that, contrary to Fleming et al's assertion, diarrhoea in each age group, and not only young children, has a seasonal pattern, despite the differences in incidence. Osika and Muganwa-Kamya comment on the representativeness of our patients. Our efforts focused on the appropriate selection of controls so that biases were avoided and the relative risks were estimated as accurately as possible. Nevertheless, the cases that we included in the study were comparable for age, fever, history of contact, and geographical distribution with the cases reported in the Sentinelle system during the same period. The predictive value of vomiting in diarrhoea due to Norwalk-like viruses seems to us debatable.[3] The hypothesis that some cases may be attributable to person to person transmission by index cases contaminated by infected shellfish does not contradict our results. We stated only that most cases of acute diarrhoea are not due to consumption of shellfish. From our results we calculated the maximum proportion of cases of acute diarrhoea attributable to consumption of raw shellfish to be 3.4% (95% confidence interval 0% to 10.8%), which is not significant.

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1 Flahault A, Garnerin P, Chauvin P, Farran N, Saidi Y, Diaz C, et al. Sentinelle traces of an epidemic of acute gastroenteritis in France. *Lancet* 1995;346:162-3.
2 Valleron AJ. Six ans de surveillance épidémiologique des diarrhées aiguës en médecine générale francaise. *MTP* 1998;1:55-63.
3 Blacklow NR, Greenberg HB. Viral gastroenteritis. *N Engl J Med* 1991;325:252-64.

Physical dependence on zopiclone

Prescribing this drug to addicts may give rise to iatrogenic drug misuse

EDITOR—I was interested to read Jones and Sullivan's report on dependence on zopiclone[1] because Ruben and I had earlier reported six cases of misuse of zopiclone among polydrug users in Liverpool.[2] The average daily dose of zopiclone was 105 mg (range 90-380 mg) and the average duration of use 10 months (6-24 months). All the patients initially used the drug for sleeping but later developed tolerance to its sedative property. Common daytime side effects reported were drowsiness, dry mouth, nausea, ataxia, and psychomotor slowing. The main withdrawal symptoms reported were rebound insomnia, a feeling of being edgy, and a strong craving 6-8 hours after the last dose. This led to self treatment to obtain relief from the withdrawal symptoms. None of the patients injected the drug, primarily because they did not know that it could be injected. All the patients were previous misusers of temazepam and preferred zopiclone because it did not cause amnesia as temazepam did. Two patients even forged prescriptions to obtain the drug.

Worryingly, all the patients admitted to knowing many other addicts who were misusing zopiclone and said that it was growing in popularity among addicts as a safe and strong sedative. The misuse of zopiclone is yet another example of iatrogenic misuse of a drug marketed enthusiastically by its manufacturers as a non-addictive substitute, as has been reported for carisoprodol[3] and buprenorphine.[4] Zopiclone is being prescribed as a sedative freely by both general practitioners and psychiatrists. Awareness of its potential for misuse needs to spread, and caution needs to be exercised in its prescription.

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2 Sikdar S, Ruben SM. Zopiclone abuse among polydrug users. *Addiction* 1996;91:285-6.
3 Sikdar S, Basu D, Malhotra AK, Varma VK, Mattoo SK. Carisoprodol abuse: a report from India. *Acta Psychiatr Scand* 1993;88:302-3.
4 Singh RA, Mattoo SK, Malhotra AK, Varma VK. Cases of buprenorphine abuse from India. *Acta Psychiat Scand* 1992;86:46-8.

Risk of dependence may be greater in those with dependent personalities

EDITOR—Jones and Sullivan report the potential of zopiclone to give rise to dependence.[1] The risk of dependence (physical and psychological) may be greater in those with dependent personalities. Our series highlights this possible association.

Case 1—A 60 year old woman with schizophrenia and a history of dependence on temazepam and slimming tablets was admitted for alcohol detoxification. Zopiclone 7.5 mg was prescribed for insomnia with good effect. At follow up she had remained abstinent from alcohol but complained of anxiety, with tremors, palpitations, and apprehension, which were relieved by 22.5 mg zopiclone daily.

Case 2—A 40 year old man admitted with recurrent depression and a dependent personality was prescribed zopiclone 15 mg at night for insomnia. After discharge he reported an increase in his symptoms of anxiety, which were relieved if he took 15 mg zopiclone every morning; his symptoms worsened when the drug was not available. Three days after zopiclone was stopped he was readmitted with extreme anxiety, insomnia, and panic attacks .

Case 3—This 71 year old woman had a 45 year history of hospital admissions for depression. During the latest five month admission she received zopiclone 7.5 mg (subsequently increased to 15 mg) at night for insomnia. After a review of her drug treatment when this was stopped she reported severe insomnia, feeling very anxious, difficulty in breathing, and palpitations. She also threatened to end her life if the drug was not reinstated.

Although in each case zopiclone was initially prescribed for insomnia, the patients adjusted their doses themselves to relieve symptoms of anxiety during the day. When zopiclone was stopped they experienced withdrawal symptoms: tremors, palpitations, panic attacks, and rebound insomnia.

These cases suggest physical and psychological dependence to zopiclone in patients of different age, sex, and diagnosis. In common was the fact that they had dependent personalities. Although personality may increase the risk of psychological dependence,[2] it would not fully explain physical symptoms.[1 3] As zopiclone has some anxiolytic effect we suggest that cautious assessment is required, noting anxiety and personality, before it is prescribed. Short term use with adequate monitoring may also reduce the likelihood of dependence.

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1 Jones IR, Sullivan G. Physical dependence on zopiclone: case reports. *BMJ* 1998;316:117. (10 January.)
2 Priest RG, Roberts A. Hypnotic drugs: not quite as bad as we thought. *Prescriber* 1995;6:62-4.
3 Thakore J, Dinan TG. Physical dependence following zopiclone usage: a case report. *Hum Psychopharmacol* 1992;7:143-5.

Doctors need to know more about advance directives

EDITOR—Doyal has written about the need for education and training of doctors about advance directives.[1] Because of the Law Commission's proposal to give advance directives a statutory basis[2] doctors must understand the legal implications.[3] The BMA has published a code of practice to guide health professionals,[4] but uncertainty still exists. At the BMA's conference in Brighton in 1996 a motion proposing that "advance directives should be specific and clearly defined" was defeated. Little has been published about doctors' knowledge of advance directives; in a survey of general practitioners in South Australia, where advance directives have legal force under the Natural Death Act 1983, Ashby et al showed that lack of knowledge was an obstacle to the effective use of living wills.[5]

We conducted a postal survey in Dorset of doctors' knowledge of advance directives. We sent a 10 item questionnaire (copies available from us) to 80 hospital doctors (22 registrars, 58 consultants) based at four NHS

trusts and to 80 general practitioners. Forty two (53%) hospital doctors and 47 (59%) general practitioners replied, with an overall response rate of 56% (89/160). None of the NHS trusts had a policy on advance directives, and only three general practitioners worked in a practice with a policy.

Seventeen (40%) hospital doctors and 21 (45%) general practitioners had not heard of the BMA's publication *Advance Statements About Medical Treatment*. Few respondents (20) knew that patients in a persistent vegetative state could have treatment withdrawn only by the court. Only 44 respondents knew that an advance directive was invalid in the case of a mentally ill patient receiving treatment under the Mental Health Act. Fifty six respondents would have voted in favour of the motion proposed at the BMA's conference in 1986. Twenty seven (64%) hospital doctors said that they would be able to discuss advance directives with a patient, but only 20 (47%) said that they would be able to assess a person's mental capacity to make one; the corresponding figures for general practitioners were 40 (82%) and 37 (76%) respectively. Most (76) respondents indicated that they would like more guidance about advance directives.

The message is clear: the medical profession needs to address how best to inform and train doctors about advance directives.

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1 Doyal L. Advance directives. *BMJ* 1995;310:612-3.
2 Law Commission. *Mental incapacity*. London: HMSO, 1995. (Law Commission No 231.)
3 Gadd E. Changing the law on decision making for mentally incapacitated adults. *BMJ* 1998;316:90. (10 January.)
4 BMA. *Advance statements about medical treatment*. London: BMJ Publishing Group, 1995.
5 Ashby M, Wakefield M, Beilby J. General practitioners' knowledge and use of living wills. *BMJ* 1995;310:230.

Rescue thrombolysis may work even though primary thrombolysis has failed

Editor—Gershlick and More discuss the new therapeutic options for myocardial infarction and suggest that patients in whom thrombolysis fails should receive rescue angioplasty.[1] We challenge this view and propose that rescue thrombolysis might be considered in cases in which primary thrombolysis has failed.

Two trials have compared rescue angioplasty with conservative management, with equivocal results.[2,3] One trial randomised patients with an occluded artery three hours after the onset of symptoms; it detected a non-significant reduction in mortality in the angioplasty group compared with the group managed conservatively (1/16 *v* 4/12).[2] A similar study with 73 patients who were managed conservatively and 78 who had angioplasty showed that rescue angioplasty performed more than 4.5 hours after the onset of symptoms failed to yield any effect on mortality or on resting ejection fraction.[3] Furthermore, it leads to high rates of reocclusion, and an unsuccessful procedure is associated with a high mortality.[4]

These data suggest that rescue angioplasty has no proved benefit. Even if it is shown to be superior, its availability is limited and in most instances time consuming transfer to a centre with interventional facilities is needed. On the basis of the literature and our own experience we favour rescue thrombolysis instead. A recent trial in 37 patients with acute myocardial infarction with persistent electrocardiographic changes after treatment with streptokinase compared the effect of rescue tissue plasminogen activator with that of placebo. Rescue thrombolysis resulted in a significantly smaller infarct and better ejection fraction.[5]

Our department runs a home thrombolysis programme, which uses a telephone based electrocardiogram system. The electrocardiogram of symptomatic patients is transmitted from the patient's home to the hospital and anistreplase is given on the spot by the ambulance service if it is indicated. We considered rescue treatment with thrombolysis or angioplasty if the ST segment did not return to normal after 90 minutes of treatment. The decision for either treatment was left to the attending doctor. From 1993 to 1997, 51 patients were given rescue treatment (table). Mortality was similar with rescue angioplasty and thrombolysis but the extent of the myocardial infarction, as estimated from measurement of maximal serum creatinine kinase, was smaller with rescue thrombolysis. Repeated thrombolysis was safe and there was no excess of major bleeding (1.7% *v* 2.6%).

These data suggest that rescue thrombolysis is at least as effective as rescue angioplasty and should be considered when primary thrombolysis has been unsuccessful.

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Results of carrying out angioplasty or giving rescue thrombolysis in 51 patients in whom thrombolysis initially failed

	Rescue angioplasty (n=12)	Rescue thrombolysis (n=39)	P value
No (%) with anterior acute myocardial infarction	7 (59)	23 (39)	NS
ST shift (mm)	26.3	28.6	NS
Mean (95% CI) maximal serum creatinine kinase (U/ml)	2445 (236 to 5403)	1491 (336 to 3319)	0.006
No (%) who had died at 21 days	1 (8)	6 (15)	NS

1 Gershlick AH, More RS. Treatment of myocardial infarction. *BMJ* 1998;316:280-4. (24 January.)
2 Belenkie I, Traboulsi M, Hall CA, Hansen JL, Roth DL, Manyari D, et al. Rescue angioplasty during myocardial infarction has a beneficial effect on mortality. *Can J Cardiol* 1992;8:357-62.
3 Ellis SG, da Silva ER, Heyndrickx G, Talley JD, Cernigliaro C, Steg G, et al. Randomized comparison of rescue angioplasty with conservative management of patients with early failure of thrombolysis for acute anterior myocardial infarction. *Circulation* 1994;90:2280-4.
4 Ellis SG, Van de Werf F, Ribeiro-daSilva E, Topol EJ. Present status of rescue coronary angioplasty: current polarization of opinion and randomized trials. *J Am Coll Cardiol* 1992;19:681-6.
5 Mounsey JP, Skinner JS, Hawkins T, MacDermott AFN, Furniss SS, Adams PC, et al. Rescue thrombolysis: alteplase as adjuvant treatment after streptokinase in acute myocardial infarction. *Br Heart J* 1995;74:348-53.

Quality of life assessments may help some patients

Editor—Greenhalgh's story of how her patient felt exploited by the quality of life assessments in a clinical trial she had been invited to join highlights a common misconception held by a distressing number of clinicians.[1] Many clinicians seem to feel that inquiring in an objective and standardised way about a patient's social, emotional, and occupational wellbeing during treatment somehow increases anxiety, is unacceptable to patients, and may affect recruitment into clinical trials.

We would like to leaven this story with a little fact. A survey conducted by two of us into the acceptability of quality of life assessments among patients with early breast cancer found that virtually all participants found these questions acceptable.[2] Only 2 out of 102 patients found the semistructured psychiatric interview upsetting, and only 1 out of 102 found some questions embarrassing. Patients were not only willing to participate in the study, but found the experience helpful.

Indeed, in a current clinical trial using extensive quality of life assessments, only 1 out of 140 patients declined to participate; most participants commented that the trial was an excellent opportunity to express their anxieties and concerns.

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1 Greenhalgh T. None of our business? *BMJ* 1998;316:482. (7 February.)
2 Fallowfield LV, Baum M, Maguire GP. Do psychological studies upset patients? *J R Soc Med* 1987;80:59.

Integrated care pathways increase use of guidelines

Editor—The article by Campbell et al on integrated care pathways is timely.[1] Recently several publications have suggested that pathways are a useful tool to promote effective clinical practice.[2,3]

Clinical effectiveness requires not only the development of evidence based guidelines but also that they are used in routine clinical practice. Many more guidelines are written than are implemented.[4] The reasons

for this are complex. They include lack of awareness of national guidelines, the need for prompts at the time of clinical decision making, and feedback on the effect of changes in practice.[5] Guidelines developed by expert panels often fail to consider the views of the many professionals caring for patients, resulting in a lack of local acceptance. Standardisation of care has been shown to improve outcomes, but arbitrary variations in clinical practice continue to occur.

An integrated care pathway determines locally agreed multidisciplinary standards based on evidence, where available, for a specific patient group. Development of the pathway includes a critical evaluation of all aspects of current clinical practice and review of the available evidence.[1] This allows the development of locally agreed guidelines, which are incorporated into the pathway and provide the standard for future routine patient care. Guidelines are more likely to succeed if they are developed by those who will be using them.[4] Adherence to guidelines is also facilitated as the pathway forms part or all of the patient's record and is available for review when clinical decisions are being made.[5] Documentation of the reasons for variation from the pathway further encourages compliance.

Analysis of the causes of variation provides valuable information which can be used to improve clinical practice.[5] It also allows clinicians to evaluate the effectiveness of national guidelines at a local level and to collect observational evidence when randomised trials are impractical or unjustified.

The rapid increase in the use of pathways supports the recognition that they can provide a powerful tool to facilitate the implementation of locally agreed multidisciplinary guidelines to promote effective clinical care.

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1 Campbell H, Hotchkiss R, Bradshaw N, Porteous M. Integrated care pathways. *BMJ* 1998;316:133-7. (10 January.)
2 West E, Newton J. Clinical guidelines: an ambitious national strategy. *BMJ* 1997;315:324.
3 Kitchiner D, Bundred P. Integrated care pathways. *Arch Dis Child* 1996;75:166-8.
4 Delamothe T. Wanted: guidelines that doctors will follow. *BMJ* 1994;307:218.
5 Thomson R, Lavender M, Madhok R. How to ensure that guidelines are effective. *BMJ* 1995;311:237-42.

Lumbar puncture should not be delayed in subarachnoid haemorrhage

Editor—Wasserberg and Barlow's article is a timely reminder of the important position that lumbar puncture still has in diagnosing subarachnoid haemorrhage.[1] Although we endorse their general conclusions, we disagree about the timing of lumbar puncture after subarachnoid haemorrhage.

The authors suggest that lumbar puncture should be deferred until 12 hours after the onset of headache to look for xanthochromia. However, fresh blood may be seen in the cerebrospinal fluid shortly after the onset of subarachnoid haemorrhage, even in patients with normal computed tomographic appearances. In fact, nearly half of patients with ruptured aneurysms with blood stained cerebrospinal fluid will not have xanthochromia.[2] There can, therefore, be no justification for delay.

Imposing an artificial restriction of six to 12 hours for a diagnostic lumbar puncture will only delay the further management of these patients. In many UK centres this management consists of early transfer to the neurosurgical unit for further investigation and, if the patient is in a good clinical state, surgery within hours of diagnosis. Any unnecessary delay in the treatment of these patients exposes them to additional risks of re-bleeding, which most commonly occurs within a few hours of the initial ictus.[3]

If computed tomography gives negative results in a patient with a suspected subarachnoid haemorrhage, the patient should have lumbar puncture immediately, irrespective of the timing from the ictus.

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1 Wasserberg J, Barlow P. Lumbar puncture still has an important role in diagnosing subarachnoid haemorrhage. *BMJ* 1997;315:1598-9. (13 December.)
2 MacDonald A, Mendelow AD. Xanthochromia revisited: a re-evaluation of lumbar puncture and CT scanning in the diagnosis of subarachnoid haemorrhage. *J Neurol Neurosurg Psychiatry* 1988;51:342-4.
3 Fuji Y, Takeuchi S, Sasaki O, Minakawa T, Koike T, Tanaka R. Ultra-early rebleeding in spontaneous subarachnoid haemorrhage. *J Neurosurg* 1996;84:35-42.

Doctors must understand terminology used to describe psychological therapies

Editor—The increasing provision of counselling after traumatic events has attracted unhealthy cynicism from the public about the calibre of those providing the service and the apparent lack of coping ability of those perceived as needing support. This is compounded by misunderstanding of the counselling profession and its many subspecialties.

The paper by Moynihan et al on the the evaluation of adjuvant psychological therapy in patients with testicular cancer adds to this confusion. It uses the terms "psychotherapy," "counselling," and "adjuvant psychological therapy" interchangeably and does not distinguish between these therapeutic approaches, which differ widely in their theoretical underpinnings and application in practice. The last paragraph dismisses the benefits of counselling for patients with cancer on the basis of a study that used adjuvant psychological therapy, which is a form of cognitive behaviour therapy.

Counselling and psychotherapy are often used generically to embrace the broad range of psychological interventions, but "counsellor" is also often used synonymously with "information giver," whereas "psychotherapist" would not be. The paper states that "counselling is often presented as an integral part of sound medical practice; … this may be creating a split between the care of the mind and body by healthcare professionals, who may think that their duties of informing and reassuring patients should be passed to a counsellor." Here the confusion between counselling and giving support and information requires clarification. Counselling as an intervention is offered by trained, qualified counsellors, while support and information may be given by a range of people using counselling skills.

The paper also reflects the difficulties, evident throughout the psychological literature, of evaluating psychological interventions, determining effective means of assessing emotional needs, and measuring outcomes of counselling, which by definition aims at subjective end points. For instance, some patients respond to an existentialist counselling approach, seeking to understand the meaning of the cancer in the context of their lives and belief systems, while others prefer a cognitive behavioural approach, which focuses more on solutions. Adjuvant psychological therapy may not have been the most appropriate therapy for all patients in this study, and a low response rate should be interpreted with caution.

Improved understanding of the role and practice of counsellors is important for the lay public, but more so for professionals. It is vital that those involved in both the physical and psychological care of patients with cancer take responsibility for informing themselves of the psychological subspecialties and applying the correct terminology.

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1 Moynihan C, Bliss JM, Davidson J, Burchell L, Horwich A. Evaluation of adjuvant psychological therapy in patients with testicular cancer: randomised controlled trial. *BMJ* 1998;316:429-35. (7 February.)

Improving management of diabetes in residential and nursing homes

"Home clinic" facilitates communication with carers

Editor—Tattersall and Page highlighted the problem of providing diabetes care for patients in residential and nursing homes.[1] In Hull we have attempted to tackle the growing problem of unstructured diabetes care in these establishments with the development of our "home clinic service." [2] This service started in 1991 to provide

structured diabetes care in the community by secondary care staff.

We had identified a small but increasing number of patients who attended the hospital based clinic but gained little from the consultation. A high proportion of this group lived in nursing or residential homes. They commonly required hospital transport, which prolonged the hospital visit, and usually had a relative or a professional carer providing day to day diabetes care and support. However, the carers rarely accompanied the patient on the hospital visit. Accurate information was often unavailable to assist the consultation, and the only reliable method of relaying changes made during the consultation back to the carer was by letter.

Referrals to our home clinic service are currently made only by consultant diabetologists, although some general practitioners have recently shown interest in purchasing this service for housebound patients with diabetes. The protocol encompasses all annual review procedures, which are carried out by a diabetes specialist nurse who visits the patients' residence. Screening includes a foot examination, for which training was provided by the diabetes centre podiatrist. The nurse also arranges eye screening through a domiciliary visit by an optometrist or attendance at the ophthalmology department. Control of glycaemia is assessed and problems are discussed with the carer. The home visit improves communication and means that any changes to treatment can be made immediately and treatment goals can be discussed.

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1 Tattersall R, Page S. Managing diabetes in residential and nursing homes. *BMJ* 1998;316:89. (10 January.)
2 Taylor CA, Hicks D, Norman A, Smith AC, Walton C. Home clinic service. *Diabetic Med* 1994;2(suppl 2):S7.

British Diabetic Association publishes guidance

Editor—Tattersall and Page rightly draw attention to the lack of organised care for elderly people with diabetes living in residential and nursing homes.[1] Given the large numbers of elderly people with diabetes, the increased risk of their admission to hospital, and the need for more visits by general practitioners[2] the argument for improving the quality of care is irrefutable.

Simple measures such as ensuring that there is a link person in each home, educational input from a diabetes specialist nurse, and help for general practitioners to provide regular systematic care following locally agreed guidelines could have great effects. The British Diabetic Association has recently published a new edition of *Recommendations for the Management of Diabetes in Primary Care*,[3] which would be helpful to those in primary care developing their own protocols. We also publish *Diabetes Care Today—a Guide for Residential and Nursing Home Managers and Staff*.[4] The association is preparing a guidance document specifically aimed at the needs of people with diabetes in institutional care, and this should be available later this year.

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1 Tattersall R, Page S. Managing diabetes in residential and nursing homes. *BMJ* 1998;316:89. (10 January.)
2 Benbow SJ, Walsh A, Gill GV. Diabetes in the institutionalised elderly: a forgotten population? *BMJ* 1997;315: 1868-70.
3 British Diabetic Association. *Recommendations for the management of diabetes in primary care*. London: BDA, 1997.
4 British Diabetic Association. *Diabetes care today—a guide for residential and nursing home managers and staff*. London: BDA, 1994.

Discrimination against gay and lesbian doctors goes against GMC's guidance

Editor—As co-chairs of the Gay and Lesbian Association of Doctors and Dentists (GLADD) we were concerned about an anonymous letter published in Career Focus several months ago.[1] It was by a gay doctor in a training post and raises several important issues.

Firstly, as the writer pointed out, gay and lesbian doctors in the armed services are liable to dismissal for no other reason than their sexuality. This happened to one of our members in 1997 after he was "outed" by a tabloid newspaper.

Secondly, doctors in the armed services are required to report service personnel whom they know to be homosexual even if this information has been acquired during a clinical consultation and in the knowledge that the person concerned will be dismissed. In any other context such breach of confidentiality could amount to professional misconduct. This issue is relevant to all doctors as it erodes the confidence of gay and lesbian patients in the profession as a whole and is clearly inimical to good medical practice. We believe that immediate action is required by the government, the armed services, and the General Medical Council.

Thirdly, some gay and lesbian doctors in training in the NHS are still reluctant to be open about their sexuality for fear of discrimination by colleagues. We can offer them some comfort by pointing to the General Medical Council's guidance to all doctors, which states: "You must not discriminate against your colleagues, including doctors applying for posts, because of your views of their lifestyle, culture, beliefs, race, colour, sex, sexuality or age."[2] We recognise, however, that practice may be very different.

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1 Not coming out. *BMJ* 1997;classified suppl:2. (13 December.)
2 General Medical Council. *Duties of a doctor. Good medical practice*. London: GMC, 1996.

Levonorgestrel intrauterine device can be left in place for five years

Editor—Mansour and Guillebaud have drawn attention to the effective duration of use of the levonorgestrel intrauterine device.[1] The Medicines Control Agency has reviewed clinical data on the product authorised in the United Kingdom (Mirena). After advice from the Committee on Safety of Medicines the licensing authority has now said that the period for which the device can be inserted can be extended from three years to five years.

The two studies that Mansour and Guillebaud cite as supporting the device's efficacy over five years relate to a previous formulation of the device not marketed in the United Kingdom.[2 3] Pharmacokinetic data suggest that the rates of release of levonorgestrel by both the old and the new formulations are similar over five years, so these data are supportive.

The updated product information for the levonorgestrel intrauterine device will allow a duration of use of five years and state that the device has a pregnancy rate of less than 1 per 100 woman years. Clinicians and users may be reassured that this method of contraception is effective when left in utero for five years.

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1 Mansour D, Guillebaud J. Data support extended use of levonorgestrel intrauterine systems. *BMJ* 1998;316:l671. (30 May.)
2 Sivin I, El Mahgoub S, McCarthy T, Mishell DR Jr, Shoupe D, Alvarez F, et al. Long term contraception with the levonorgestrel 20 mcg/day (LNg20) and the Copper T 380 Ag intrauterine devices: a five year randomised study. *Contraception* 1990;42:361-78.
3 Sivin I, Stern J, Coutinho E, Mattos C, El Mahgoub S, Diaz S, et al. Prolonged intrauterine contraception: a seven year randomised study of the levonorgestrel 20 mcg/day (LNg20) and the Copper T 380 Ag IUDs. *Contraception* 1991;44:473-80.

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